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Original Communications.

BELLADONNA VERSUS OPIUM; ADMINISTRATION OF CHLOROFORM DURING SLEEP; UNUSUAL NUMBER OF WORMS.

NOTES FROM PRACTICE, BY J. SCHNECK, M.D., MT. CARMEL, ILL.

IN his valuable paper in the EXAMINER of February 15th, 1874, on "The Antagonistic Effects of Belladonna and Opium," Dr. Wardner, after having examined the literature on the subject, deprecates the fact that those cases in which the results have been unfavorable, have not been published. He has been able to find but one reported case (that of Dr. J. P. Chesney) of opium-poisoning, in which belladonna was given as an antidote, that has terminated unfavorably, the subject in this case being an infant.

However, in the same number of the EXAMINER, under the head of "Transactions of the Chicago Society of Physicians and Surgeons," are two other fatal cases of opium-poisoning reported in which belladonna was given, one by Dr. Etheridge, and another by Dr. Wilder. In both cases the antidote was given until the physiological effect (dilatation of the pupils) was produced.

The following case, it is thought, will be of interest in this connection, as adding a fourth fatal case:

BELLADONNA VERSUS OPIUM.

Oct. 9, 1871, at 5 p. m., was called to see W. G——, physician, aged near forty years; of nervous temperament; had for years been an immoderate user of alcoholic stimulants. Upon my arrival at his office (less than a square's distance from my own), found him in profound coma, breathing stertorously, deeply suffused condition of the skin, pupils very small, sensibility almost entirely gone, consciousness rapidly failing, but, after severe shaking and loud talking, aroused him sufficiently to tell me indistinctly, "I have taken thirty grains of morphine; I did it on purpose; I knew what I was doing." A note was found on his table containing a similar statement,

where were also found an empty morphine bottle and a goblet. Consciousness and sensibility now rapidly disappeared. From what I can learn from his brother, the poison had not been taken over thirty minutes when I first saw him. After all efforts to produce vomiting had failed, galvanism, artificial respiration, stimulants, rubefacients, etc., were diligently applied. In addition, hypodermic injections of atropiæ sulphas were given at intervals of thirty minutes, until full dilatation of the pupils was produced, which effect was produced by about 7 p. m., two hours after he was first seen, three injections having been given. This effect was kept up during the next seventeen hours by two subsequent injections, the total amount of atropia given being about one and one-half grains. During the time from the eighth to the tenth hours after the poison was taken, there appeared to be improvement, sensibility and consciousness partially returning, but not sufficiently for conversation. After this time he again grew worse, and at 12 m., Oct. 10th, he died, nineteen hours after the morphine was taken. Although the atropia in this case did not save life, yet I think the improvement during the time from the eighth to the tenth hours is plainly to be accredited to it.

ADMINISTRATION OF CHLOROFORM DURING SLEEP.

April 28th, 1873, was called to see J. W——, a rugged-framed but anæmic boy of five years. Examination revealed a large polypus of the vesicular variety in the left naris. Upon making an attempt to extract it, he became so badly frightened that I had to desist. Called the next morning at six o'clock, and found him asleep. I rolled a handkerchief into the shape of a bird's nest, in which I poured about one drachm of chloroform; this was at first held about six inches from the mouth and gradually brought nearer until total anæsthesia was produced. I now extracted the polypus, and did not even appear to disturb him of his sleep.

UNUSUAL NUMBER OF WORMS.

Was called Feb. 13th, 1875, to see R. W——, a healthy farmer boy fourteen years old, slender form. Was given the following history: He had been attending school during the past three months; when out of school, had been doing such work as is usually done on a farm during the winter season; that he had during this time felt well; had been working hard all this day until about 5 p. m., when he was attacked with pain in the bowels, nausea, and finally vomiting. I saw him at 8 p. m., and found him some easier, but still complaining of pain in the abdomen, which, on examination, I found to be very hard and uneven to the touch. Otherwise could detect nothing wrong. Left him seven powders, each consisting of two grains each of santonin and calomel; four to be given this evening, one every two hours; at 6 the next morning a full dose of castor oil. The remaining three powders to be given two days afterward in the same way.

Feb. 18th. Heard from my patient to-day, and he is reported by his father, a man of undoubted veracity, to have passed 188 round worms (ascaris lumbricordes), all of good size. The patient says: "I am feeling as well as usual, and feel as if I was much taller."

THE RECURRENCE OF MEASLES.

By James I. Tucker, A.M., M.D., Chicago.

THE epidemic of measles prevailing at the present time, of which Chicago comes in for her full share, affords an opportunity to observe the disease in many of its phases. Among these phases is its recurrency. It has been my privilege to attend a number of cases, and I have been surprised at the frequency with which I have been told (when, upon hearing the irritative cough, I have expressed my suspicions of measles) that this could not be the case, as the patient had already had them. But in every instance my suspicions have been confirmed. I make due allowance for the inaccuracy of the laity in matters of this kind, and recognize the possibility of mistakes on the part of medical men, but now and then a case occurs in the experience of individual practitioners, which so fully confirms the observation of others that it comes with the force of conviction.

Such a case is that of Carrie S——, aged ten years, whom I treated for measles in July, 1874. This was in every respect a typical case, in its prodromes, preliminary stage and exanthem. I am attending the same girl, who is now, March 23, 1875, abed with measles, this case differing from the other in no essential particular, the temperature in both cases rising to 103.2° F.

Instances of this kind are perhaps only worthy of mention, because the best authorities differ with regard to the possibility of a recurrence of measles. Even Rosenstein and Willan, who had the most ample opportunities, and devoted the closest attention to eruptive disease, state that they have never seen a case—at least one attended by fever. Probably second attacks are very rare, and differ in different epidemics, in this respect to be classified with variola, varicella, scarlatina, etc. Doubtless many cases are simply a re-appearance of the eruption after a temporary suppression. But if all cases were recorded, wherein occur two outbreaks in a single patient, at intervals of months or years, under the eye of a single practitioner, it might be found that such occurrences are more frequent than is generally supposed, and thus their authenticity established beyond a doubt.

Third attacks are probably very infrequent, but sometimes occur, as in Van Dievan's case (recorded Ziemssen, vol. 11, p. 43).

AN INTERESTING SURGICAL CASE.

Reported by H. W. Sawtelle, M.D., of the Marine Hospital Service.

PRIVATE George T. Cottrell, Co. G, 1st U. S. Sharpshooters, aged 21 years, was wounded while in the act of firing his piece, at the battle of Chancellorsville, Va., May 2, 1863, by a crusidal ball entering about half an inch above left clavicle, and about one inch from its sternal extremity,

and passing transversely between the trachea and œsophagus, lodged beneath the clavicle near the point where the subclavian emerges. He was conveyed to Washington, D. C., and admitted to St. Aloysius Hospital May 7th, 1863.

There was but little blood lost. A numb pain supervened, which lasted six months, referred particularly to the elbow joint and fingers, the fingers remaining semi-flexed three months. He was unable to speak aloud for two weeks, and nourishment could only be taken in liquid form through a tube for several weeks. The forearm was carried at right angles, and narcotics were exhibited to relieve pain. By the middle of July following, the wound had perfectly healed, and never re-opened. At this date the joint, which had become firmly fixed at right angles from inaction, was straightened while the patient was under the influence of chloroform, and frequent flexion and extension subsequently restored fully the use of the joint.

He was returned to duty, in the Veteran Reserve Corps, Oct. 31, 1863, from whence he was discharged from the service Sept. 14, 1864, and pensioned.

The hand and fingers continued to be very sensitive to cold and heat, and at times intensely painful.

One morning in the fall of 1865, he found that the power of supporting the head was lost to such an extent, that he was unable to rise from his bed, and clonic contractions, drawing the head to the right shoulder, lasted four days, but did not recur.

Late in December, 1869, the pain at the point of lodgement of the missile increased, and January 31st, 1870, Professor N. S. Lincoln, M.D., having placed the patient under the influence of chloroform, cut down and removed the ball, which was found thrust in between the subclavian and a branch of the brachial plexus, the missile resting on the artery just where it emerges from beneath the clavicle, with the nerve drawn tightly across the ball in front. Upon pushing aside this nerve in order to remove the ball, vigorous contraction of the limb ensued. The wound healed readily.

The limb is now equal to its fellow in size and strength, and with the exception of a very slight sensitiveness of the fingers to cold and heat, which is improving, the patient is entirely relieved of his suffering.

ANTI-PYRETIC TREATMENT OF ACUTE RHEUMATISM.—The Mount Sinai hospital has adopted the treatment of acute rheumatism by the use of cold externally applied. The method consists in the use of cold baths, combined with ice-bags, to the inflamed joints. Every patient does not bear well the cold baths, but the ice-bags always prove grateful and always remove the pain. The very curious point has been noticed, that, if blankets are placed over the patient, or in any way the patient be allowed to sweat, the cold loses its efficacy. It is found also that, if the ice-bags are removed from the inflamed joints, the pain sometimes re-appears, and, when it does, a return to the ice-bags again relieves the patient.

Translations.

RECENT PROGRESS OF OPHTHALMOLOGY IN FRANCE.

By Fred. J. Huse, M.D.

I. The Pupil as a Guide During Administration of Chloroform. II. The Ophthalmoscope as a Means of Diagnosis in Acute Phthisis. III. Cystic Degeneration of Conjunctival Mucous Glands.

I.

M. BUDIN made, in behalf of M. Coyne and himself, a second communication to the Society of Biology in Paris, at the session of Feb. 6th of the present year, upon the pupillary phenomena in asphyxia, and upon the difference between these phenomena in the anæsthesia from chloroform and in the anæsthesia from asphyxia.

He proceeded to state that ever since the first appearance of etherization, the subject of asphyxia has been agitated; respecting this, it was sufficient to refer to the works of MM. Sedillot, Faure, Vulpian, P. Bert and Claude Bernard. That there are manifest in the pupil differences between anæsthesia from chloroform and anæsthesia from asphyxia, he and his colleague were prepared to demonstrate. In the former condition, when it was total, it may be observed that there is contraction with immobility of the pupil. In asphyxia, on the contrary, there is at first a condition of the pupil intermediate between dilatation and contraction, then a very wide dilatation at the time of the appearance of the convulsive phenomena.

When the anæsthesia is attained, excitations fail to produce any modification of the state of the pupil. These phenomena may readily be observed by introducing into the trachea of a dog a canula furnished with a stop-cock, so that the administration of the anæsthetic may be exactly regulated according to the wish of the experimenter.

M. Schiff, however, as well as MM. Bert and Moreau, called in question the result of the experiments already briefly published in this journal.* In giving chloroform to dogs, he had very readily caused their death without having produced pupillary contraction. With chloral, M. Schiff had obtained, it is true, consequent upon an intravenous injection, a contraction of the pupil with total anæsthesia; but when, by means of a sack, he gave chloroform to the animal thus chloralized, he observed re-appearance of the dilatation of the pupil. Chloral, according to his opinion, differed in its action from chloroform; chloroform, moreover, failed in his observations to produce contraction and immobility of the pupil.

MM. Budin and Coyne demonstrated in the following manner that

*Medical Examiner, vol. XV., page 542, and *Chicago Journal of Nervous and Mental Disease*, vol. II., No. 1.

M. Schiff, by administering chloroform by means of the sack, produced in reality not anæsthesia, but asphyxia:

They first injected into the veins of some dogs nearly half a drachm of chloral, sufficient to maintain total anæsthesia during fifteen minutes or more. When each animal was completely anæsthetized they caused him to breathe chloroform vapor freely in the open air; nevertheless, the pupil remained throughout the duration of the experiment (an hour), very firmly contracted and immobile.

Having then performed tracheotomy, they inserted into the trachea of a dog a canula provided with a stop-cock, and when, 45 grains of chloral having been injected, total anæsthesia, accompanied with a contraction of the pupil that became punctiform, appeared, they closed the stop-cock. Asphyxia supervened, and with it a progressive dilatation of the pupils, which at length were very large. Upon opening the stop-cock, the pupils returned to their previous condition; again closing it, they dilated as before.

Thus, *without chloroform*, and with *pure asphyxia*, they reproduced the observations of M. Schiff. Very probably it is owing to some similar defect in the operatory process that contradictory results have been obtained by certain oculists, and by Youngken and M. Maurice Perrin.

As M. Claude Bernard has clearly shown, when the administration of chloroform is mismanaged at the outset, there is a mixture of true anæsthesia and asphyxic anæsthesia. Otherwise, neither chloroform nor ether act as asphyxiants, their vapors exerting upon the nerve centres a direct medicinal influence. All these and many others of the observations of that eminent physiologist of the College of France are confirmed by the experiments of MM. Budin and Coyne.

These authors, however, are far from claiming that asphyxia is the most frequent cause of death in the administration of chloroform and ether; these causes are multiplex, and among them, as MM. Perrin and Lallemand have demonstrated, the most noteworthy, on account of frequence, is syncope.

Asphyxia usually appears in connection with anæsthesia under certain special conditions in the method of administration of these anæsthetics. With other anæsthetics, however, this will not hold true, since it is asserted by MM. Jolyet, Blanche and Claude Bernard (for denial see reports of the session of the Surgical Society of Paris held 3d March, 1875) that nitrous oxide acts invariably as an asphyxiant. It appears that such also is the case with the nitrite of amyl, as M. Bourneville has observed the same pupillary phenomena which appear in simple asphyxia.

In brief, anæsthesia attended with asphyxia causes pupillary phenomena distinctly different from those of simple anæsthesia from chloroform. It is highly important (Sedillot, Claude Bernard) to avoid asphyxia during the administration of chloroform.

Fnally, if in the careful maintenance of anæsthesia with chloroform there appears during the stage of complete insensibility a state of average dilatation, or indeed of any dilatation of the pupil, in place of the usual contraction, the surgeon should at once assure himself whether any cause of asphyxia is influencing the anæs-

thesia.—*Progres Medical*, No. 7, Feb. 13, 1875.

II.

M. Bouchut recently had, in a case of acute phthisis, simulating typhoid fever, a striking verification of the usefulness of the ophthalmoscope as a means of medical diagnosis, in the advocacy of which he has been one of the earliest and most prominent of the French school, and concerning which his recent communications merit careful attention.*

The case, which is reported in detail† by an interne of the hospital, was that of a girl aged 13 years, who, two weeks before her entry into the hospital, was suddenly seized with a violent pain in the left side, the sense of oppression produced being so intense that two days before her entry she had several attacks of suffocation. There was some headache; a lack of appetite; no diarrhœa or vomiting.

At the time of her admission the most prominent appearance was that of general typhoid condition, with dyspnœa. Obliged to maintain an upright posture, she was breathing painfully at the rate of 70 respirations per minute. The features bore a most manifest expression of extreme debility, there being notable emaciation of the entire body. General hyperæsthesia existed, which was slightly diminished over the thorax—a symptom which was persistent, and upon which special stress is placed in the review of the case—the child crying out at the least pressure upon any portion of its body. The frontal headache caused few complaints. The mind was clear, responses to questions being correct and without hesitation. There was no trouble of the urinary organs. The cardiac sounds appeared normal.

The left anterior wall of the chest yielded upon percussion a slightly more obscure resonance than that of the opposite side, while its elasticity seemed also diminished. In front and on the right side the elasticity and sonorousness were quite normal. Auscultation on the left side revealed, just under the clavicle, a slight, fine sub-crepitant rale; during forcible inspirations and after attacks of coughing, there were some puffs of distant sub-crepitant rales. To the right and in front the vesicular murmur possessed its normal degree. Behind, on the left the respiration was distinct and without any pathological indication; on the right the unfolding of the alveoli was less distinct, the expiration appeared somewhat *soufflante*, and sometimes sub-crepitant rales could be distinguished. The resonance was normal over the entire back of the chest; voice unchanged; temperature 103°; pulse 140. The pupils were dilated and their susceptibility to light was impaired. There was no complaint of any impairment of vision.

Such were some of the more prominent symptoms of a case which was at first diagnosed by M. Bouchut himself as a double pneumonia of a bastard, unusual form. The following day, diarrhœa having set in, there was fresh hesitation and an opinion of its being typhoid fever of a thoracic variety; but the local symptoms were so widely at variance with the general condition, that on the third day M. Bouchut proceeded to make an ophthalmoscopic examination. The pres-

* Medical Examiner, Jan. 15, 1875. *Medical Times and Gazette*, Jan. 23, 1875. *N. Y. Medical Record*, March 27, 1875.

† *Progres Medical*, Feb. 27, 1875.

ence of several tubercules upon both choroids was readily observed by means of the ophthalmoscope, thus affording the indisputable diagnosis of miliary tuberculosis.

Subsequently the thoracic symptoms increased, the rales became more and more marked in intensity as well as extent, and fifteen days after admission the patient succumbed.

At the autopsy, the lungs were found to be *crammed* with the small miliary granules; scarcely a dozen, however, could be found on the peritoneum and intestines, though the mucous membrane, especially of the small intestine, was congested. There were about twenty granulations on the pia mater, and in the left sylvian fissure several were united in a small purulent collection of the size of a pea.

The eyes were divided perpendicularly to their antero-posterior diameter, and disclosed a normal condition of the refracting *media*. Upon carefully raising the retina, the choroidal pigment was found intact, and on each side of the optic nerves there were some whitish, opaque and slightly protuberant points of the size of the head of a small pin. In the right eye there were three of these very near the nerve, and three others at a slight distance. In the left eye there were two in the vicinity of the nerve, and eight others disseminated over the choroid.

While it may perhaps be too much to say, with Cohnheim,* that in miliary tuberculosis the choroid invariably contains tuberculous deposits, still, on the other hand, there is reason to believe, according to M. Bouchut, that such a condition exists more frequently than is generally thought to be the case, and in such a disease as acute phthisis its discovery is of the greatest importance to all parties concerned.

Berliner Klin. Wochenschr. No. 6, 1867.

III.

An interesting case of cystic degeneration of the mucous glands of the conjunctiva has recently been described by Dr. Carlo de Vicentiis, in which the conjunctiva presented yellowish irregular nodules upon its tarsal portion of the size of the head of a very small pin, only slightly prominent beyond the general level of the mucous membrane. These nodules were more abundant upon the upper lid than upon the lower, and in the interspaces the conjunctiva was thickened, and also smooth and shining.

A vertical section made through this conjunctiva, after hardening it in alcohol, disclosed, under examination by the microscope, numerous cavities, which were even recognizable with ease by the naked eye, and of which some of the smaller were aggregated together in groups, while others of larger size were isolated. Only a small proportion of them were furnished with an excretory conduit. Their walls were irregular, constituted of fibrous tissue, and they possessed cavities containing the degenerated products of secretion. These contents were composed of granular material and a substance which occurred in yellow scales. The granules were most abundant in the large cavities, while in the smaller ones the scales preponderated.

Dr. Vicentiis considers these cavities as conjunctival mucous glands or glandular cells that have undergone a considerable dilatation and present three stages of degeneration: the

first, that found in the smallest, still filled with epithelium intact; the second in those of medium size, containing altered epithelium in the form of scales, and finally the granular detritus found in the largest ones.*

*See, however, *Diseases of the Eye*, by Soelberg Wells, 1870, pages 48 to 51.

This lesion is attributed by the author to the retention of the products of secretion, either on account of too much secretory activity or by reason of obstruction of the excretory conduits.—*Movimento Medico-Chirugico di Napoli*, and *La France Medicale*, 13th March, 1875.

THE INNERVATION OF THE HEART AND THE CARDIAC MEDICAMENTS.

By Professor G. See.

Translated from La France Medicale, 1874, Nos. 39, 43 and 57, by H. M. Bannister, M. D.

THERE is no subject at once more interesting and more complex than that of the innervation of the heart. This study, difficult as it may appear to you, is yet absolutely necessary, in order that you may understand the importance and the mode of action of the agents which I have called the cardiac medicaments.

As regards its structure, the heart is a striated muscle, but it differs from the other muscles of the life of relation in the fact that it possesses in its substance its own centres of innervation and of action. This peculiar character merits a special study of the medicines that affect it, and only this consideration justifies the autonomy of the therapeutic group of which I propose to treat.

As a muscle, that is, as a motor organ, the heart is controlled by two principal groups of medicines, namely:

1. Those which paralyze the motor nerves (curare, calabar bean, hemlock, aconite).

2. Those which paralyze the muscular system (salts of potash, veratria).

3. Farther, the heart possesses a system of irrigating canals which do not play a merely passive part. The vessels of the heart itself, the coronary arteries, possess an active attribute, that of contractility, which itself is under the control of the vaso-motor nervous system; therefore, any agent exercising a paralyzing influence on the vascular nerves may modify in a special manner the central organ of the circulation. Therefore, a third group of agents, the vascular medicines (belladonna, bromide of potassium, ergot, nicotine, etc.), may be considered as indirectly acting upon the heart itself.

4. Finally, there exists a last group of agents which appear to have an elective action on the heart, and which, for this reason, merit the title of direct or true cardiac medicaments, that I have given them.

Before studying the mode of action of this latter class, I think it may be advantageous to review the general subject of the physiology of the heart in regard to those points on which we have the most concise and positive information.

The innervation of the heart comprehends three centres of action:

1. The first of these is situated in the heart itself and constitutes its *automotor centre* or autonome. It is formed by little nervous ganglia situated in the sinus of the vena cava and in the walls of the left ventricle.

These ganglia contain in themselves

the whole motor principle of the heart. There are those who have, in recent times, gone so far as to deny absolutely any other motor. Legallois was the first, at the beginning of the present century, to give out the opinion that the spinal cord might act upon the heart, and that there should be for this organ other auxiliary nervous centres. The means employed by Legallois to demonstrate the existence of these auxiliary centres were very primitive. His theory was, nevertheless, the correct one, notwithstanding the fact that Magendie and Claude Bernard disputed it. It belonged to a young German physiologist, Von Bezold, to place beyond any doubt the facts suggested by Legallois, and to demonstrate in an irrefutable manner the action which the cord exercises upon the heart. Von Bezold, in his beautiful experiments on the cardiac poisons, divided the nerves which go to the heart and preserved only a filament, starting from the grand sympathetic between the two thoracic ganglia; he observed:

1. That the pulsations of the heart became much less frequent.

2. That the vascular tension became greater.

3. That the issue of the blood from the arteries became more abundant and more forcible.

The cord, therefore, contains an auxiliary centre of action exercising its influence on the heart. But, by what way does the cord act on the heart? Nervous fibres leave the cord, passing by way of the thoracic nerve, according to some, by the pneumogastric, according to others.

Outside of this auxiliary spinal centre, there exists still another that acts indirectly on the heart—the *vasculo-motor* or *vasculo-nervous* centre. The vaso-motor nerves leave the spinal axis with the motor roots of the second cervical pair, and apparently have their origin in the upper part of the cord, and probably in the medulla. When this centre is affected it produces, as it were, a vascular paresis, characterized by a dilatation of the vessels and a lowering of the arterial tension. Thiry and Ludwig, in cutting the thoracic nerve, have observed the same phenomenon that Von Bezold noted in his experiment. After the section of the cord and of the cervical sympathetic, they noticed a notable lowering of the arterial pressure, and a diminution in the number of the pulsations. This last result, although contrary to the assertions of Professor Marey, ought to be considered as real and well-founded.

From these short physiological premises it results, therefore, that there are three cardiac centres of action:

a. An automotor centre (intracardiac ganglia).

b. An auxiliary spinal centre (spinal cord).

c. An indirect centre (vaso-motor nerves).

But these three centres are not alone sufficient to produce the regular play of this organ. There is still needed, in order to insure its marvelous harmony, a regulating agent, a sort of moderator that can prevent the troublesome effects of the influences we call moral, which singularly modify the contractions of the heart.

This regulator is the pneumogastric nerve, which seems to act in opposition to all the other nerves. In fact, we know that when we divide a motor nerve we immediately paralyze the muscles to which it goes, but when we cut the pneumogastric nerve we accelerate, as it were, the pulsations of the heart, instead of causing them to cease. The same nerve, when excited, instead of augmenting the action of the cardiac muscles, has the effect, on the contrary, to suspend its action.

The pneumogastric dominates, so to speak, over all the innervation of the heart; it is more powerful than the automotor centres; it governs, directs, arrests or counteracts them. The whole is subjected to its control; it is a ruler, a despot; or rather, it plays the same part as the curb or governor in the steam engine, as I have, following this analogy, already proposed for it the designation of governor of the heart, a name uni-

versally adopted at the present day. This depressing, or rather, suspensive action of the pneumogastric, admitted by the majority of physiologists, has nevertheless been denied by Prof. Schiff, and more recently by M. Onimus. Both refuse to admit the action of the vagus as a nerve of arrest, without denying altogether the retardation of the heart produced by the excitation of this nerve; but they seek to explain this effect, the one by the intensity of the excitation of the currents employed, the other by the action of related currents.

Besides the pneumogastric nerve, there is a second system of arrest, situated in the heart itself. There is, in fact, in that organ a nervous ganglia acting in a directly contrary manner to the others (the automotor centres), that I mentioned before. The physiological action of this inhibitory ganglion has been recently revealed to us by the singular properties of a new agent, studied by M. Prevost, of Geneva; I refer to muscarine. When the heart of an animal is taken from the chest, as is well known, it continues to pulsate for a few seconds, although separated from its nerves and nervous centres, on account of the nerve power it receives from its little inherent and automotor ganglia. But muscarine arrests the movements of the heart, when thus taken from the chest and still palpitating. This effect can only be explained by the action that muscarine exercises on the centre of arrest situated in the heart itself.

What is the connection between this intracardiac centre of arrest with the pneumogastric? In the actual condition of science it is difficult to answer this query, and it seems to us advisable, notwithstanding the ingenious hypotheses that have been proposed, to leave the question still in suspense.

The two systems of arrest I have described are not the only ones that control the heart; there is, in addition, an intimate correlation between the contractions of this organ and the cardiac circulation itself. It appears that everything connected with the heart has its centres of action and of arrest.

The vaso-motor system of the heart is provided with a special nerve, discovered by Ludwig and the brothers Cyon. This discovery obtained for the two last named physiologists the grand prize in experimental physiology decreed by the Academy of Sciences in 1867. The nerve of Cyon ordinarily rises by one root from the trunk of the pneumogastric, and the other from the superior laryngeal. It is separate in the rabbit and the dog, but in man is included with the trunk of the vagus nerve.

Starting from its origin in the upper part of the neck, the nerve of Cyon descends along the carotid artery by the side of the cervical filament of the sympathetic, which it accompanies without uniting with it. Reaching the chest, it anastomoses with fibres from the first thoracic ganglion, and is soon lost in the substance of the heart, or better in the dense cellular tissue situated between the origins of the aorta and the pulmonary artery.

Such, in a few words, is the anatomical description of this singular nerve which plays so important a part in the circulation by its reflex action on the motor nerves of the blood-vessels.

For the purpose of experiment, we expose and divide it in the middle portion of the neck. While the electric excitation of the peripheral portion of the divided nerve produces no pain and has no effect on the manometric pressure or on the heart, the excitation of its superior and central portion is painful and causes a depression of five or six centimetres in the manometer applied to the carotid —in a word, an enormous diminution of pressure in all the vessels. The mechanism of this reflex action on the blood pressure is shown in first paralyzing the muscular system with curare and then cutting all the nerves that go to the heart. The lowering of the pressure remaining, in this case, the same, renders it certain that it is not through the muscular system nor

through the heart that it is brought about. But if we perform the section of the splanchnic vaso-motor nerves, this vascular depression fails to appear, and therefore we have the secret of its mechanism. The excitation of this sensitive cardiac nerve reacts, therefore, exclusively on the vaso-motor nerves, producing a repletion of the heart and a diminution of the blood pressure. To the direct paralyzing influence of the pneumogastric on the heart is added also a paralyzing reflex influence of the sensitive or depressor nerve of the heart. The nerve of Cyon is therefore a moderator or depressor nerve — a nerve of arrest of the vascular centre.

En resume, there exist three principal centres of action for the heart, and three corresponding antagonistic centres constitute its system of arrest.

1. An automotor centre of action (intra-cardiac ganglia) and an intra-cardiac centre of arrest.

2. An auxiliary medullary centre (spinal cord) and a corresponding nerve of arrest (the pneumogastric nerve).

3. An indirect centre (vasculo-nervous) and a special depressor nerve of the vaso-motor system (nerve of Cyon).

The physiological premises I have just laid down were a needed preface to the study of the cardiac remedies. How, indeed, can we understand the mode of action of a medicine on the heart if we do not know beforehand the functional endowments with which that organ is provided? Would we not be continually exposed to error, if, guided only by a blind empiricism, we were ignorant of the manner in which the medicine we prescribe ought to act? It is not sufficient to know that the drug enfeebles the contractions of the heart and diminishes the frequency of the pulse, we ought yet to know how it produces these effects. Is it the cardiac fibre itself that it acts upon, or rather are the nerves of the heart first influenced by the agent? In this latter case we have still to inquire whether we have produced, for example, an excitation of the nerves of arrest or a paralysis of the automotor centres.

In order to reply to these and like queries, and to answer the questions that every physician, jealous of his art, should propose, the previous study of the mode of action of a medicine is absolutely indispensable, and consequently experimentation, "*in corpore vili*," is needful, and should serve as the basis of the therapeutic application.

But as we advance in this study, new difficulties present themselves before us. The physiological deductions and the experimental results are far from giving us all the elements of so complex a problem. Medicines are like men, and we may apply to them the celebrated saying of Pascal: "There are no two of them that resemble each other. The more we advance, the more are we able to establish the distinctions which separate them and the shades of difference between them." But it is for us to discover, at the same time, the analogies and the differences that present themselves to us in this general study of the cardiac remedies.

There are two kinds of cardiac medicines: the one acts all at once and directly upon the heart; digitalis is the type of this kind. All the effects of this agent are the result of its primal action on the heart. The other kind acts principally on the other organs, and their action on the heart is only consecutive and secondary. These are the indirect cardiac agents; such, for example, are calabar bean, aconite, veratria, etc.

Allow me to say a few words on these two last-named medicines, as regards the special point of view which now occupies our attention.

Aconite, Aconitia.—Aconite had its day of splendor in the last century, in the hands of Stoeck, who recommended its employment in acute articular rheumatism. After his death the remedy fell to the rank of the anti-nervines. It seems even to have almost fallen into oblivion, when more recently, the homœopaths on the one hand, and on the other, Dr. Schroff,

in Germany, the author of a very interesting treatise on pharmacology, brought it again into notice, and physiology itself comes to its aid in securing for it the recognition it deserves.

All the facts concur to show that aconite is a poison to the heart. I pass in silence the various effects produced by it on sensibility and motility, for here we are only concerned with its action on the heart. Administered in a rather large dose, aconite causes a retardation of the cardiac contractions and a diminution in number of the pulse beats. This action is very prompt, and death ensues from failure of the heart, before the lungs are at all affected. But let us attempt to penetrate farther in the study of its action, and first of all we must point out the different results according to the dose employed. When we give aconitia in an almost infinitesimal dose, $\frac{1}{8}$ of a milligramme (=.003 gr.), we observe a very noticeable acceleration of the pulse and the circulation. This preliminary effect is of short duration, for this action of aconite on the centres of excitation of the heart is ephemeral and transient.

As soon as this first phase of excitement has passed, we observe a singular state of fatigue of the heart, which shows itself first by a weakness of the ventricles and then of the auricles. It appears that the heart (pardon the expression) can flap but one wing, and, in fact, it only beats with the auricles, and at last stops, in a sort of tetaniform contraction. One of the factors of the inhibitory system ought to be found super-excited in order to produce this effect. But what is this inhibitory system of the heart? According to what I have said before, the cause must be sought in the pneumogastric nerve or the ganglia at its termination. It is the curb that is over applied, and the arrest takes place; the poison takes effect on the vagus nerve, and the heart undergoes a tetaniform contraction.

The excitation of the suspensive or inhibitory system becomes weaker, and finally ceases, while the convulsive contraction still continues. At the moment when the inhibitory excitement is exhausted, how is it that the heart remains in its, as it were, tetanized state? It is because aconitia is a nervo-muscular poison; it acts not only on the nerves, but on the cardiac muscle itself. We may sum up the phases of aconite poisoning as follows: In the first place we observe an acceleration of the pulsations of the heart, due, without doubt, to an excitation of the automotor centre.

In the second stage, there is a retardation of the pulse and the cardiac contractions, probably produced by an excitation of the nerve of arrest (pneumogastric).

In the last stage, we have arrest in tetanic contraction by exhaustion and paralysis of the suspensory system, and finally inertia of the muscle and paralysis of the intra-cardiac motor system.

Veratria.—Veratria offers some analogies with aconite, but it presents numerous differences as regards the considerable modifications it produces in the arterial pressure. It is in virtue of this special property that it may be considered as one of the most powerful anti-pyretic agents.

We find in this case, as in that of aconitia, a preliminary stage, which is the more pronounced when the veratria has been given in very weak doses. When we give 1 milligramme (=.015 grs.) of veratria, we augment instead of diminishing the peril. In fact, instead of retarding the pulse by the use of a small dose, we only accelerate it. This is a new and striking proof of the importance of this study, and of the difficulties with which it bristles. Therefore, we ought to take special care lest we employ a timorous and dangerous moderation in the choice of the dose we administer; for that which may seem to be an excess of prudence, in reality is only a blind temerity; because the action of veratria in too minute doses may produce the most deplorable results.

Veratria should be employed in the

dose of from six to twelve milligrammes (= .09 to .18 grs.), in order to produce really useful therapeutic effects; that is, a considerable retardation of the contractions of the heart. Little by little, this retardation continues, even until the inhibitory system loses its excitability and its power. The pneumogastric nerve becomes exhausted in its trunk much sooner than in its branches, as the following experiment will prove:

Take an animal, administer to it veratria; we then observe a notable slowing of the pulse; next cut the pneumogastric, and the pulse becomes quickened; excite the peripheral portion of the nerve, and the pulsations again immediately become slower. At last, after a certain time, the heart itself is affected, as we have shown in the case of aconitia, and the irritability of the cardiac fibre itself becomes extinct.

We have stated that veratria exercises a very interesting influence on the vascular pressure: When we give it in the dose of about ten milligrammes, we observe, simultaneously with the enfeeblement of the pulse, a considerable lowering of the blood pressure, a diminution of the contraction of the heart, and a manifest reduction of the temperature.

But if we recall what has been said in regard to the nerve of Cyon, we find an important application of these physiological results. We are aware that if we cut this nerve and irritate its central portion we cause a centripetal excitation that diminishes the pressure throughout the whole arterial system, and in particular in the mesenteric system.

This nerve, therefore, counteracts the working of the vasculo-motor centre. The pneumogastric is a centrifugal, and therefore a motor nerve. The excitation of its peripheral portion corresponds in its effects to the irritation of the central end of the nerve of Cyon.

In all the experiments performed on the pneumogastric, we may note, simultaneously with the slowing of the pulse caused by the excitation of this nerve, a diminution in the vascular pressure; but the synchronism between the two phenomena is not complete and absolute; the sphygmograph shows, in reality, a diminution of the arterial tension previous to the retardation of the pulse; the two phenomena are parallel, but not connected.

However it may be, we find the physiological action of the nerve of Cyon in the effects of veratria on the vascular tension, and see by this how useful the application of experimental physiology may be to the study of the action of drugs.

In a second phase of the action of veratria the lowering of the vascular pressure becomes still more pronounced, and after a certain time the vascular walls present an extreme depressibility; the vessels and the heart itself are so far paralyzed that further excitation is impossible. In this third period the heart is affected, and the individual dies in a state of extreme weakness, of profound collapse, as is observed in certain cases of pneumonia treated with veratria.

We should therefore bear these facts in mind, and not fail whenever we administer this remedy, to attend to auscultation, and to watch with the greatest care the state of the heart and pulse during the whole time we employ this remedy, generally so efficacious, sometimes so dangerous, and always so delicate and difficult to direct.

* * * * * * *

Our attention has so far been particularly given to two of the agents which deserve to be placed in the foremost rank among the indirect cardiac medicaments: aconite and veratria, the one paralyzing the motor nerves of the heart, and the other acting on its muscular fibre. By the side of these we may place curare, calabar bean, hemlock and nicotine in one group, while the salts of potassium should be placed in a second by the side of veratria. All these agents diminish the excitability of the inhibitory system of the heart, but they employ different means to arrive at that result.

1. The one kind paralyze the terminal extremities of the nerves, the pneumogastric in particular (conium, curare); both of these two poisons affect the motor nerves of the life of relation before they act on the heart itself.

2. Others, before suppressing the functions of the organ itself, excite its inhibitory system (first period of nicotine and calabar bean poisoning).

3. Within five years, two experimenters at Dorpat, Dr. Oscar Schmiedeberg and Richard Koppe, have noted the singular effects of a new alkaloid obtained from poisonous mushrooms, and in particular the agaricus muscarius, whence the name muscarine, which they have given to the extract, and which was the subject of a very recent communication to the Societe de Biologie by Dr. Prevost, of Geneva.

This young and talented physiologist has shown that by injecting muscarine into the veins of an animal, we arrest the heart in diastole. The action of this poison is so much the more curious since in such a condition the heart is not dead, and is even not paralyzed, for we can revive its contractions after long hours of inaction. Such an effect can therefore only depend on the irritation of a nerve of arrest. Is it the pneumogastric that is here in action? Evidently not, since the previous section of this nerve, in an animal poisoned with muscarine, does not hinder the effects of the poison. It must, therefore, be the intracardiac centre of arrest itself on which muscarine acts.

Physostigmia, applied to man, causes a more or less considerable retardation of the heart's pulsations; but we do not yet know with certainty its real mode of action. When we act on a muscarinized heart (if I may be excused the expression) with physostigmia, we break in upon the silence of that organ which, before arrested in diastole, now commences to pulsate. We may therefore conclude that physostigmia acts on the heart in manner contrary to that of muscarine. As to conia, it produces an effect upon the heart only very slowly. Indeed, hemlock, in this respect, like curare, paralyzes all the motor nerves before attacking the central organ of the circulation; it is very probable that this poison acts, in causing the stoppage of the heart, by exciting the pneumogastric nerve.

In opposition to these last-named agents, belladonna and its derivative atropia, paralyze the cardiac extremity of the vagus. Their effects are therefore altogether the reverse of the preceding. The acceleration of the heart pulse in this case can only be attributed to a paralysis of the pneumogastric, or of the intracardiac centre of arrest; in a word, this effect is the direct opposite to that of muscarine.

It would be very interesting to pursue this study, and to be able by this physiological method to recognize as many special medicines as there are functions or particular systems in the cardiac organ. Unfortunately, such an analysis is barely sketched, and we cannot present here more than a distant and imperfect outline.

There still remains to be considered an agent the most important, and at the same time the best known. I refer to digitalis. Its history seems altogether exhausted; it appears as if nothing new could be added after the numerous memoirs of which it has been the subject. Nevertheless, all has not been said that may be said on this interesting topic. Many lacunæ remain to be filled, many dark points to be cleared up, many obscurities to be done away with.

We rank digitalis among the direct-acting cardiac agents, but we should bear in mind that its mode of action is none the less very difficult to explain. Two different methods have been followed to resolve this difficult problem.

On the one hand, we may perform the experiments on ourselves in a very simple fashion. The homœopaths first adopted this primitive procedure. The celebrated Hahnemann first of all experimented on chamomilla. He noticed, after the ingestion of this medicine, which, nevertheless, seems

very inoffensive, a feeling of strangulation, of cephalalgia, numbness in the lower limbs, hallucinations, and confused and strange ideas. (There is no doubt but that the experimenter had no need of taking chamomilla in order to experience such effects.) If I mention these first attempts in experiment, it is solely for the purpose of putting others on their guard against similar errors. But it is in this manner that the effects of digitalis have been studied by two patient but fanciful physicians, Sanders and Stadion. Let us see how they have described them. In the first few days that followed the ingestion of the drug, they noted nothing in particular, but about the sixth day nausea and anorexia appeared. From this time on the trial *in anima medicarum*, became, as we can easily understand, very much interfered with. The subjects of the experiment ceasing to eat, excreted no more urea, and the results became false and erroneous.

Stadion and Sanders, after the eleventh day of their experiment, fell into a state of extreme feebleness, which is easily comprehended after so long a period of involuntary fasting. One of them himself assured me that he was so weak as to be unable to maintain an erect position. But let us leave these courageous but mistaken investigators, whom I only mention in order to point out the imperfections of their method.

All those who have made themselves the subjects of their experiments ought to have kept the conditions the same both before and during their investigation; this is where they have failed. A few, however, have sought to ward off this source of error. Voit has carried on a very remarkable series of researches on the effects of foods and medicines, by ascertaining very accurately the state of the ingestions and that of the excretions, both before and during the experiments.

Dr. Mejoin, on his part, has not only weighed the quantity of food ingested in his experiments, but he has also indicated the quality. In taking fifteen hundred grammes of food, we are able to tell just how much carbon and nitrogen it contains.

Voit fixed the amount of the expenditures, that is, the excretions, in order to be able to make out the complete physiological balance, and it is only at that price that he is able to lay down any really scientific conclusions.

In short, all the experiments on digitalis have been made in the most imperfect and coarsest manner. We can see in what respects they are faulty: "The first are altered by the loss of appetite which inevitably ensues a few days after the ingestion of digitalis; while those made by the second method are rendered erroneous from the want of a previous establishment of the exact balance in the organism.

In the presence of these obstacles in the way of experimentation, the imperfections of the methods employed, and the want of exactitude in the results obtained, we may understand the difficulties with which this study is surrounded. I will now try to infuse a little order and light into this chaos.

I. *Effects of digitalis on the heart and blood-vessels.* In addition to the researches of Sanders and Stadion, I ought here to notice the interesting monographs of Bordier, Legroux, and Lelion, among the more important works undertaken in late years on this subject.

Sanders has stated that when he took a moderate dose of digitalis, he observed during the first few days a notable acceleration of the pulse. The fundamental action of the drug followed this initial effect, namely, a retardation of the pulse, but while this retardation existed the slightest cause produced again a quickening.

With a large dose, the retardation appears, says he, in three hours, and attains its maximum at the end of twenty-four hours; if we continue this dose for five or six days, the pulse becomes small, irregular and intermittent. If it is necessary to purchase the slackening of the pulse at the expense of an acceleration that lasts two or three days, we are in danger of making false steps. But

this initial acceleration fortunately has not been noticed by all the observers, and particularly by the young experimenters whose names we have given above. They have obtained the retardation of the pulse at once, and in a very satisfactory manner.

What is the cause of this retardation?

In order to explain it one eminent physiologist has practiced experimenting with an artificial circulatory apparatus, in which the heart was represented by a hollow bulb of india rubber, tubes of the same material taking the place of the vessels. By the aid of this instrument, ingenious no doubt, but assuredly imperfect, he has been enabled, he states, to formulate a very curious law: "When the calibre of the discharging tubes is contracted, the india rubber bulb can empty itself only with difficulty. If, therefore, the heart drives out the blood it contains with difficulty, we ought to find an augmentation of the pressure in the vessels, and reciprocally the increase of the intravascular pressure produces the slowing of the pulse" (Marey). I give this proposition in Prof. Marey's own words, without comment on my own part.

Experiments on Animals. We will now examine the results furnished by physiological experiment. It is necessary that we should exercise a choice in the animals which we would submit to experimentation. Some resist the poisons which are fatal to us; others are violently affected by those which are without effect upon ourselves. With the exception of the Guinea pigs, all the herbivora are resistant to the effects of digitalis, and it is necessary to have recourse to very powerful doses in order to produce appreciable results. On the carnivora, on the other hand, digitalis acts in a very marked manner.

Let us take a small dose of digitalis. I do not speak of digitaline, which differs according to the mode of preparation (Nativelle, Homolle, and Quevenne); the powdered digitalis is the preferable preparation. If, therefore, we inject a small quantity of the infusion into an animal, we observe almost immediately a slowing of the pulse. Increase, then, the dose (five or ten centigrammes of the solution), and we obtain the directly contrary effect, the heart commences to gallop. If you wish to produce this acceleration, you have only to employ the strong dose at first. It is therefore useless to make an algebraic equation with the words digitalis and retardation of the heart. If we inject a decidedly toxic quantity, we arrest the heart in systole. Thus we see that whenever we employ digitalis, we find ourselves, as it were, between two fires: on the one side the acceleration of the pulse, and on the other the arrest of the heart's action through excessive retardation. * * * *

The first effect of digitalis is exercised on a special nervous apparatus, while its later action takes place on the cardiac muscular apparatus itself. By this rule, the different action of the drug, according to the dose employed, should be explained.

We have seen the effects of digitalis as regards the retardation of the pulse, but it yet remains to us to study its effects on the vascular pressure. It is admitted, as a general rule, that digitalis augments the blood pressure, but it seems to me needful to establish here an important distinction: When we administer the medicine in therapeutic doses, the slackening of the pulse and the augmentation of the pressure progress *pari passu;* but if we give a stronger dose we have first an increase of blood pressure, and shortly after a more or less considerable decrease of the same. If, on the other hand, we administer a toxic dose, we obtain a diminution of a quarter or a third in the intra-vascular pressure.

These singular effects of digitalis may be summed up in the following synoptical table:

I. WITH THE SMALL THERAPEUTIC DOSE.

1. Retardation of the pulse.
2. Increase of the intra-vascular pressure.

II. WITH LARGE DOSE.

1. Acceleration of the pulse, which may terminate in a retardation.

2. Decrease of blood pressure, perhaps changing to an increase.

III. WITH A POISONOUS DOSE.

1. Sudden slowing of the heart, which is attacked in its muscular substance.

2. Permanent lowering of the blood pressure.

Digitalis is a singular poison, distinguished from most of the others by what we may call its cumulative action; when the doses rapidly follow each other their effects accumulate. Very few drugs possess like cumulative action. Very small but repeated doses may result in a sudden explosion of its dangerous effects. We insist upon this point, since this drug, at the same time so wonderful and so terrible, ought never to be given without a previous knowledge of these possible results.

There is finally a last point to which it appears useful to give our attention: that is the change in the contractile force of the heart, and in the quality of the pulse. Under the influence of digitalis, while the heart beats slower, its pulsations become more energetic, and perhaps it is the increased force of the contractions that produces the elevation of the blood pressure.

On the other hand, the pulse becomes dicrotous, it beats double, and if we cast our eye on the sphygmographic trace of the radial artery, we perceive that this dicrotism is on the descending line. This descending line only returns to its point of departure every two or three pulsations; in short, it fails to reach it, and we have then two or three ventricular systoles following each other before the heart succeeds in accomplishing one diastole. We find sometimes in the human subject that the pulse becomes double or triple, in a word, polycrotous. We ought to carefully take note of this disturbance in the rhythm of the pulse, as this last phenomenon is very frequent; it is met with even in cases where the digitalis is given in therapeutic doses, and when these relatively insignificant doses are kept up during a certain time.

Inversely, we employ digitalis to cause the disappearance of similar phenomena of arythmy depending on a disease of the heart, and which offer a very striking resemblance to those caused by the drug in a healthy individual.

RELATIONS OF ATMOSPHERIC ELECTRICITY AND OZONE TO DISEASE.—Dr. Geo. M. Beard, *Popular Science Monthly*, in a valuable discussion of the above topic, gives the following practical suggestions:

1. Many nervous and other diseases, and very many nervous sensations, are perceptibly affected by changes in the quantity of electricity and ozone. After eliminating the factors of heat and cold, of moisture and dryness, of carbonic and nitric acid, of oxygen, pure and simple, there remains much that only ozone and electricity can well account for.

2. Not a few sensitive and impressible organizations experience variations of strength and debility of vigor, and malaise, that very well correspond to the variations in atmospheric electricity, or ozone, or both. From 8 to 12 A. M. is the golden time for brain work, and from 4 to 8 P. M. the silver period for all mental labor; during these two periods there is the greatest amount of ozone and electricity in the air.

3. Irregular disturbances in the electrical conditions of the atmosphere in storms, and especially in thunder storms, and in our climate north-east storms unquestionably affect the nervous system of impressible temperaments unpleasantly, and bring on or aggravate neuralgic, rheumatic and other pains, as well as incite mental distress and discouragement. Now, just before thunder storms, atmospheric electricity is apt to be negative.

Editorial Department.

ASSOCIATION OF AMERICAN MEDICAL EDITORS.

THE members of this association, and all others connected editorially with the regular Medical Press of this country, are requested to meet in the parlors of the Galt House, in Louisville, Ky., at 7:30 o'clock, on Monday evening, May 3d, 1875; being the evening previous to the assembling of the American Medical Association. The Association of Medical Editors was formed in New Orleans, in May, 1869, for the cultivation of friendly relations, consultations concerning topics of mutual interest, and the urging of concert of action in the advancement of education, both preliminary and medical.

The proposition for its formation was made by Dr. Theophilus Parvin, of Indianapolis, whose object was to make it the instrument of accomplishing much good. Up to the present time, however, the number of editors present at the several meetings has been so limited as to render it impracticable to carry into execution the original plans of the Association. We hope there will be a larger number of members present at the coming meeting. It is the duty of the President to deliver to the Association an address, and we doubt not the present incumbent of that office, Dr. W. S. Edgar, of St. Louis, will be prepared to discharge that duty on the evening named.

PUBLIC MEDICAL INSTRUCTION.

THE Medical Society of the County of Kings, N. Y., has appointed a standing committee, called the "Committee on Public Instruction." The chief duty enjoined on the committee is set forth in the following paragraph, quoted from the *Medical Record:*

"It is the purpose of the Committee of Public Instruction to communicate useful information on medical subjects to the public through the medium of the press. To this end, it shall obtain, prepare or select suitable papers, which it shall send for publication to such newspapers or periodicals (not medical) as it may select. The writer's name or other means of identification shall not accompany any original (specially-prepared) paper, nor shall the name of the author of any selected article be published when such author is a member of this or any neighboring society."

So far as relates to hygiene and sanitary regulations, such a committee, performing its work judiciously, would be of great benefit to the public in all our cities.

Correspondence.

A LETTER FROM PARIS.

EDITORS MEDICAL EXAMINER: A sufficient time having elapsed since the introduction of Koumys as a remedy in the Paris hospitals, to permit of extended observation, the profession here finds itself in a position to give a verdict upon the use of this "milk champagne" as a therapeutic agent. It has been largely experimented with by MM. Gueneau de Mussy, Gubler, Chauffard, and many others, in their hospital wards, and their researches show that koumys acts as a wonderful renovator of debilitated organisms, whatever may have been the cause of the cachexia, and that its metasyncritic action is very marked.

Though about ninety years ago, John Grieve, an English surgeon in the Russian service, called attention to the marvelous effects of koumys in all chronic diseases, the investigation was not continued, one reason being the difficulty in procuring the milk. But to-day, thanks to chemistry, we find that a mixture of asses' and cows' milk, at a certain stage of fermentation, gives about the same composition and effect.

It is in the treatment of pulmonary phthisis that a great deal of interest centres upon the koumys. The observations have been numerous in this connection, and so many cases are reported of amelioration, in every stage of the disease, that even the most skeptical have to give the subject consideration. In general the effects upon the patient may be formulated as follows: At first, a slight excitement from the alcoholic nature of the drink; this is followed by cessation of the cough, interruption of the night sweats, improvement of appetite, and marked increase in weight. In other words, general and decided improvement, and this within a few days.

To quote two cases will give an idea of the action of koumys as seen here. Dr. Labodie-Lagrave gives an observation in the *Gazette Hebdomadaire* (September, 1874), in which he states that the patient, a girl of sixteen years, was in "the last stage of pulmonary consumption." A fatal termination was expected, but under the use of koumys her strength returned, and her weight increased two kilogrammes in six weeks.

The second case that I will mention is recorded by Dr. Henri Huchard, in the *Union Medicale* for January 26th, 1875. The patient was fifty years of age, and presented the signs of pulmonary tuberculosis in the third stage (cavernous rales, etc.). For more than four months her state had been getting worse. Her weight had decreased thirty pounds. "Every day the fever was consuming her; the poor patient hardly slept, a prey to an incessant cough and to profuse sweatings; hardly any appetite, frequent vomitings, and an abundant diarrhœa had come, within a short time, to steal from the sufferer what little strength she had remaining." He ordered her

one bottle of koumys (Edwards, No. 1) every day, and, the tolerance being soon established, "the sufferer saw her appetite return, her strength increase at the same time that the night sweats, diarrhœa, vomitings, and febrile exacerbations at night disappeared." In two months' time she had gained twelve and one-half pounds in weight, her face was colored, the cough was less frequent, and the lungs showed a real local amelioration. "*This patient was, without doubt, always tuberculous, but she was no longer phthisical.*"

While conversing with M. Gueneau de Mussy on the subject of koumys, he expressed to me the greatest satisfaction with its results in private practice, but regretted that the expense (three francs per bottle) prevented its constant use in the hospitals.

It is rather a discouraging task to attempt to review, in a letter, a work so full of interesting and important facts as is the one entitled, "Lectures on Anæsthetics and Asphyxia," that has lately been published by M. Claude Bernard. The name of the author is in itself a guarantee for the value of the work, and I shall only attempt to call attention to a few observations, even here finding difficulty in making a selection.

These lectures, originally delivered at the College of France during the years 1869 and 1870, have been carefully revised for publication, and have been enriched by the additional experience of M. Bernard during the past four years.

The first ten chapters are devoted to anæsthetics, not limiting the study to those substances generally named anæsthetic, as chloroform and ether, but considering the effect upon the sensitive nerves of warm water, chloral, opium and its alkaloids. The effect of the combined action of morphia and chloroform, in man as well as in the lower animals, is studied at length. It is found that—1st. If an hypodermic injection of the chlorohydrate of morphia is given an animal recovering from the effects of chloroform, the anæsthesia returns; that is, the animal becomes again insensible. 2d. On giving chloroform to an animal previously narcotized by morphia, a much less quantity is required to produce insensibility than ordinarily. Quite a number of cases are quoted where these results have been obtained in men.

In experimenting upon the tolerance of animals for opium, M. Bernard records the following curious experience, that may give a valuable indication for the general practitioner:

Having thoroughly established the tolerance in an animal so that large doses of the narcotic are borne without inconvenience, he found that it needed but a thorough *purgation* to make a new animal; that is to say, to return the animal to his first state of sensibility to the action of the drug.

Giving chloral to an animal under the influence of morphia makes the hypnotic effect more apparent, but the excitability is not diminished as by chloroform. Administering chloroform to an animal chloralized causes the sensibility to entirely disappear. Hence the conclusion that chloral, at least under these circumstances, has an action like that of morphia.

The second part of the work, comprising eight chapters, is given to the study of asphyxia, more particularly that produced by the vapors of char-

coal. The author establishes, by his experiments and investigations, that the asphyxia from this cause results from the poisonment of the red blood corpuscles by the oxide of carbon rather than from the action of the carbonic acid.

To try and indicate the experimental research and carefully considered theories that the author advances on this subject would be but to translate the entire work, a task that others will doubtless perform, so I will finish this hasty notice by giving M. Bernard's outline of treatment for asphyxia, with which he closes the book:

The necessity for removal from the vitiated atmosphere and the value of artificial respiration he fully insists upon; but, in regard to the latter, he deprecates too violent efforts to establish respiration as tending to rupture the lung tissue, already more or less deteriorated following the time of exposure. The results of these ruptures would be sufficient in many cases to cause the death of the patient from secondary disease, as pneumonia, should he recover from the asphyxia. In extreme cases where cauterization may be used, the author mentions the regions over the trunks and branches of the cervico-brachial plexus as the best points of application; for these nerves, remaining longer impressionable, are able to act more directly upon the movements of respiration. The inhalation of pure oxygen and the transfusion of blood give good results practically as well as theoretically, but are generally too difficult of application.

Though but a comparatively short time has passed since this drug was introduced in Paris, Jaborandi (from the Pylocarpus Pinnatus), is now a familiar word in the wards of the hospitals. Its effects are very decidedly sudorific and sialagogue, and M. Gubler, in a recent report before the Society of Therapeutics, mentioned one case where a patient, to whom he gave 4 grammes of this medicament, sweated profusely and excreted "more than a quart (litre) of saliva." Occasionally it causes vomiting, if administered soon after eating, and when its effects are very marked, the patients complain of great thirst. Sometimes there appears tumefaction of the sub-maxillary glands and even a veritable ranula is formed.

M. Gubler sums up the principal indications for this powerful remedy as follows: Outside of anasarca or dropsy from any cause, where jaborandi gives good results, it is found that sub-acute articular rheumatism is greatly benefited by this drug. In bronchitis with asthma, it sometimes produces marvelous effects. In six cases M. Gubler arrested the access of the asthma by its use.

Following the course of new remedies, it is being used in all kinds of diseases, and its reported favorable action in diseases as widely separated as rebellious ophthalmia and polyuria requires the test of wider experience before the truth and limit of its action can be well determined. The subject is receiving a great deal of attention, and many of the medical journals are engaged in discussing the pros and cons of jaborandi. One thing seems sure, and that is that this drug has shown enough favorable action to insure its being received as a regular occupant of the Parisian medicine-chest.

The *Gazette des Hopitaux* has a cus-

tom of giving the *conclusions* that are arrived at by different writers, without burdening its pages with the details of the investigations, referring those who desire to more fully investigate the subject to the works themselves. Regarding this as a very happy custom, I shall imitate it from time to time. For example:

"Experimental Researches on the Mode of Action of the More Commonly-Employed Vomitives," by Dr. H. Chouppe (*Arch. de Phys.*). *Conclusions.* The mode of action of vomitives commonly used is not the same for all, and if we examine the phenomena that precede and follow vomiting, we will find very great differences. Ipecacuanha and its alkaloid, by whatever way they are introduced into the economy, excite vomiting by a direct irritation of the terminal points of the pneumogastric nerves in the mucous membrane of the stomach.

Tartar-emetic and apomorphine have double action; they act on the mucous membrane of the stomach, but also directly on the bulb. There is, however, a difference in these two medicaments. The former acts sooner upon the stomach than upon the bulb, while the apomorphine acts more rapidly upon the nerve centres. The best proof that can be given of this is that doses of tartar-emetic, sufficient to cause vomiting, should be greater when this substance is injected into the veins than when introduced into the stomach. With the apomorphine, on the contrary, it is by injecting into the general circulation that the maximum effect is reached.

C. C. MATTESON, M.D.

PARIS, March 10th, 1875.

Gleanings from Our Exchanges.

BIOGRAPHICAL SKETCH OF THE LATE DR. RUSH.

Philadelphia Correspondence of Boston Medical and Surgical Journal, April 1st, 1875.

A MOST interesting biographical sketch of the life of the late Dr. Rush, whose brilliant success in medicine will not soon be forgotten, was recently published in one of the daily journals. It was he who said, "Let my epitaph be, 'He fed fevers.'" For such was his clear-sightedness that, even in the day of starvation diet, he saw how much wiser it was to supply daily fuel to the fires of fever than to allow it to devour the body of his patient. He made enemies by this as well as by other independent and original modes of treating disease. I think a brief *resume* of this sketch will interest your readers.

Born in a village just out of Philadelphia, in 1745, Rush received his degree of A. B. at Princeton College in 1760, and, although a mere boy, was the peer of any of his fellows in all his collegiate studies. Immediately after completing his collegiate course, he entered upon the study of medicine, and was one of the ten students who attended Dr. Shippen's course of anatomical lectures, the first ever given in this country. Gifted with an investigating mind, Rush studied labo-

riously until 1766, when he sailed for Edinburgh, and, after two years of hard work, received his degree of M. D. from the university of that city. He next gave a year to continental travel, mingling with the most cultivated medical men in Paris and London. At the close of this year he returned to Philadelphia, and began the practice of medicine. At the very early age of twenty-four, he was elected Professor of Chemistry in the College of Philadelphia, and became a frequent contributor to medical and general literature. The present University of Philadelphia was at this time in process of development, lectures having been delivered at irregular intervals for a year. The acquisition of young Rush completed the faculty of the first medical school in the United States. Some fifteen years afterward, the primitive institution was merged into the present university, and Dr. Rush, recognized as one of the most brilliant young physicians of the country, held the position of professor of the institutes of medicine, and of practice, as well as of clinical medicine. He was an exceedingly minute man, garnering with care every floating fact, theory and incident for future use, absorbing everything, forgetting nothing. In 1790, after a practical experience of twenty years, he published his new principles of medicine. His views met with strong opposition. He had great confidence in the lancet, and a lasting faith in the power and utility of calomel, which he termed "the Samson of the materia medica." His opponents accepted the illustration, jocosely remarking, "It has slain its thousands." These powerful agencies, under the skillful control of Dr. Rush, were productive of beneficial results.

In 1793, Philadelphia was terribly stricken by yellow fever. It raged from July to November, creating an average death-rate of forty daily, with a final aggregate of five thousand victims. The city was panic-stricken. During this frightful crisis, Dr. Rush made herculean efforts to subdue the my, working practically a portion of his time, and devoting the remainder to a thorough scientific analysis of the disease. He visited one hundred and fifty patients weekly, and saved thousands of lives by his original and judicious treatment. His special method of handling the fever, in spite of its success, was bitterly reviled. Journalists, pamphleteers, and scurrilous anonymous writers attacked him with fierce malignity, until the discussion, primarily based on questions of professional skill, sank into petty persecution. He was stigmatized as a murderer, and threatened with expulsion from the city. As Harvey injured his professional prospects by his theories concerning the circulation, and was hooted as a common fool, so did Dr. Rush lose public confidence because he bravely turned his back upon the beaten path to subdue a pestilence heretofore never vanquished. But he moved on calmly and unfalteringly.

On the termination of the fever, a motion to thank the faculty in general, and Dr. Rush in particular, for eminent services during the epidemic, was offered at a large public meeting, but not a man in the audience was bold enough to second it, and it failed.

Dr. Rush finally turned upon one pamphleteer of bitter force and invective, who violently attacked him, and made him pay five thousand dollars for his sport. This was one of many assaults made upon Dr. Rush, but he survived them all, and built up and retained the largest practice in Philadelphia. A few years afterwards a reaction set in, soon after the gift of a gold medal which Rush received in 1805 from the King of Prussia, in recognition of his replies to certain questions touching the treatment of yellow fever. For the same reason the Emperor of Russia sent him a brilliant diamond ring, and the Queen of Etruria a gold medal.

Dr. Rush was a voluminous writer. Out of seven large volumes six were upon medical subjects. His *Medical Inquiries and Observations, Diseases of the Mind, Medical Tracts, Health, Temperance and Exercise*, gave him a

high reputation at home and honorable notice abroad. In early life he found time to study politics, not as a selfish partisan, but as an honest, noble-minded citizen. In 1776 he was a member of the celebrated Congress which signed the Declaration of Independence. In 1777 he was appointed physician-general of the military hospitals of the middle department. In 1787 he was a member of the Pennsylvania convention which ratified the Federal constitution. In 1799 President Adams appointed him treasurer of the United States Mint, solely on account of his faultless character and sterling integrity. The office was an unsolicited gift, and he held it fourteen years.

But few cities in Europe, and certainly none in this country, have such a variety of charitable institutions as Philadelphia. No one citizen contributed more to the organization of many of these than did Dr. Rush. In 1785 he planned and organized the Philadelphia Dispensary, the first institution of the kind in America. He was president of the Philadelphia Medical Society; president of the Philadelphia Society for the Abolition of Slavery; founder of the Philadelphia Bible Society; vice-president of the celebrated American Philosophical Society; a strong, practical friend of the temperance cause, and his work, *An Inquiry into the Effects of Ardent Spirits upon the Human Body and Mind*, is full of valuable information. He was a lofty-minded Christian gentleman. His private life was one of unsullied purity, and his public career unsurpassed in its many brilliant developments and practical results for the common good of his country and his fellow-men.

In my next letter I hope to give you some interesting facts concerning Dr. Keen's later experiences in the use of chloral as a preservative agent.

PHILADELPHIA, March 23, 1875.

ON THE MERITS, REAL AND COMPARATIVE, OF AMPUTATIONS IN THE LOWER THIRD OF THE LEG, WITH SUGGESTIONS AS TO THE PROSPECTIVE VALUE OF PERIOSTEAL FLAPS.

From the New York Medical Journal, April, 1875.

DR. Stephen Smith read the paper of the evening on the above subject of which the following is a brief synopsis:

Ambrose Pare, three hundred years ago, related a case of a man who had his foot shot off, and, after it had healed, had the leg amputated five finger-breadths below the knee, on account of the burden it proved to be in walking.

Pare, in commenting on this subject, says: "We must do otherwise if any such thing happens in the arm;" —and thus shows how accurately he appreciated a most important principle in modern surgery, which is as follows: *The special function of the limb should be considered, and as far as possible preserved in selecting the point and method of operation.* Unfortunately, however, if a score of limbs are examined, few, if any, will show that the surgeon was governed in the point of election of his amputation by the special functions of the limb, and the facility of adapting to it artificial appliances. It is most important to the patient that this be well considered, as it may determine whether he is to be a pauper for the rest of his days, or a self-supporting citizen. And this may best be arranged by a consultation of the operating and mechanical surgeons.

There has been much difference of opinion in regard to the relative advantages of amputation of the leg below the knee, and at the lower third, in regard to its usefulness to

the patient for the adaptation of an artificial limb. When patients were only able to obtain the wooden "pin and bucket," it was an advantage to have a short stump; but with the improvement in artificial appliances, it was found that a leg which had been amputated at its lower third could have applied to it an apparatus which would enable the patient to walk with great facility.

In our own day the principles which should govern surgeons in the election of the site and method of amputation of the leg are vague and not defined; and the results by no means sustain Pare's proposition in regard to the special function of the limb. One of the chief reasons for the diversity of surgical opinion in regard to amputations is, that not enough attention is given to the consideration of the mechanical aids which can be adapted to substitute the natural extremity; whereas, at the present time, artificial limbs have reached to such a point of perfection, that in the lower extremities they may be said to completely restore function and symmetry when the stumps admit of their proper application; and, moreover, they are sufficiently cheap to be within the unaided or aided means of all. As regards amputation of the leg, the two governing rules are: 1. Safety to the life of the patient; 2. Serviceableness of the stump. As regards the first, the chances for the life of the patient are doubled by the low operation; and, as regards the second, it depends on the point and method of the amputation.

To render a stump useful, it is necessary that it be absolutely free from tenderness, and the principal cause of tenderness is a thin, imperfect cicatrix attached to the bone. It is usually the result of sloughing, imperfect, or retracted flaps, which in themselves are produced by an operation yielding too thin coverings to the bone.

Tenderness may best be avoided by having the cicatrix situated anteriorly or posteriorly to the end of the stump, and the bone covered by a thick, fleshy mass, as practiced by Teale, of Leeds, or Lee, of London.

An important question arises here as to whether it is necessary for patients to bear their weight on the end of the stump, and the expert mechanicians answer, "Yes." Dr. Hudson, of this city, one of the best students of the mechanism of artificial appliances in this country, gives it as his opinion that a stump that will take the weight of the body is useful, whatever the method of support may be. And, if this is so, an important question that arises is, what part the periosteum may perform in this respect, when it is employed in covering the end of the bone. Since 1814 periosteal flaps have at different times been suggested, and, during the winter of 1862–'63, Assistant Surgeon G. M. McGill, U. S. A., practiced the use of these flaps to nourish the bone. His method was, to raise a section of the periosteum from the anterior surface of the bone sufficient in area to cover the cut extremity of the bone. The stumps are described as being well rounded, and capable of bearing pressure. The advantages which have been attributed to this method are: 1. That the cicatrix does not become attached to the bone. 2. That osteo-myelitis is prevented. 3. That the stump presents a rounded form and is firm.

The disadvantages are: 1. That the periosteum being poorly nourished, is liable to slough. 2. If it does become attached, osteophytes may develop, which will destroy the stump.

PHAGEDENIC CHANCROID.—The patient came into hospital suffering from a sloughing chancroid. It began as a soft chancre on the prepuce. After taking on a phagedenic character it involved and destroyed the penis, and then extended down and spread over the thigh. All treatment proved useless to stop its progress, the actual cautery proving absolutely of no avail.

NYMPHOMANIA AND VAGINISMUS CAUSED BY ANAL FISSURE.

From the Detroit Review.

IN a recent paper "On some Forms of Rectal Diseases of Women," Dr. Edward W. Jenks, of Detroit, writes as follows:

Many of the disorders to which the rectum is subject are, without doubt, of much more frequent occurrence among women than men. The association of uterine and rectal diseases is quite common, as may also be said of uterine and vesical disorders. The truth is, that the continuity of tissues, the distribution of nerves and vessels, and the intimate relationship existing between the pelvic organs, render it obvious why disorder either of function, structure, or place of one may produce analogous condition in another. To treat symptoms alone in diseases of the pelvic region, as in any other part of the body where viscera can be subjected to physical exploration, is most thoroughly unscientific. There are certain symptoms common to all pelvic diseases of women; others are peculiar to certain individual diseases. I am prompted to mention this as prefatory to a short paper upon certain forms of rectal diseases, for the reason that I have known many cases of disorders of the rectum where the patient had been treated alone for uterine disease, and *vice versa*, and I design mentioning some cases in illustration of the assertion.

[From those reported, we extract the following interesting cases of Fissure of the Anus:]

Nymphomania.—M. R., aged 20, unmarried, was sent to me by one of my former pupils as a patient that was extremely unfortunate, on account of her insatiable venereal appetite, but that it had existed only a short time, and that as yet she had not been guilty of any offence in that direction beyond masturbation. She was put in a quiet boarding-house, and under the most careful surveillance. Every symptom indicated some pelvic disorder, rather than a mental disease. Examination of the generative organs revealed no indication of their disease beyond an undue congestion of the external organs, and especially in the neighborhood of the clitoris, where there was a slight abrasion. I learned that defecation was followed by excruciating pain, and upon examining the rectum I found a broad, irritable ulcer, just within the verge of the anus. Ether was administered by Dr. Walker, when I made forcible dilatation of the *sphincter ani externum*, after the manner first suggested by Recamier. It is needless for my present purpose to follow up the details of treatment in this and the other cases reported in this paper. I will only add that it was a matter of astonishment to the few medical gentlemen familiar with the history of this patient to observe that, with the cure of the anal fissure, the nymphomania disappeared. She has since married, and when I saw her last she informed me that she was in excellent health, and happy in her new relationship.

Vaginimus.— Mrs. S., aged 34, had considered herself for some years as a sufferer from uterine disease. When I first saw her she was confined to her bed, and had been most of the time for the preceding six months. She had been told by her medical attendant that in all probability there was some disorder of the womb that was producing all her physical ailments. The local treatment advised consisted of emollient and astringent vaginal washes. Every symptom of which I could learn caused me to think there was some form of disease of the uterus or its annexes. Upon attempting to make a digital examination of generative organs, I was compelled to desist on account of the severe spasmodic pain my finger produced at the ostium vaginæ. My diagnosis of the case was vaginismus, which was further corroborated when

I learned that coitus had been for a long time impossible, and the few attempts at it had been followed by days of suffering. At this time my attention was not directed to the rectum for any symptom or complaint of the patient, but at a subsequent visit I found her exhibiting symptoms of the most intense agony, and upon inquiry as to its cause learned that her bowels had just moved, and that for more than two years, whenever she had had an evacuation of the bowels, her sufferings had been of a like character, while for the past few months they had been gradually increasing in severity.

I then attempted to make a digital examination of the rectum, but soon found that it caused such severe pain that I deferred it two days, when ether was administered. I then discovered the cause of the patient's intense suffering to be due to a broad and unusually deep fissure within the anus, together with one of those small and extremely sensitive excresences so frequently found cropping out from the mucous membranes of this locality, as associates of irritable ulcers of the rectum. I removed the small growth, and then forcibly dilated the sphincter after the usual method. The patient made a good recovery, and with the cure of the fissure there was an end to both the painful defecation and the vaginismus. The removal of the latter difficulty was apparently quite as satisfactory as the relief from physical suffering, for its existence had begun to be a cause of the most disagreeable sort of domestic unhappiness. The transition from a bed-ridden patient to a lady of active habits was quite rapid, nor did she require much further medical treatment.

THE PATHOLOGY OF THE BLOOD.—M. Laptschinsky, of St. Petersburg, contributes a paper to the *Centralblatt*, on the microscopic changes undergone by the blood in various diseases. He finds that in various diseases in which marked febrile symptoms are present, the microscopic aspect of the blood is essentially different from that of health. The changes consist in the blood-corpuscles not running into regularly formed rouleaux, but accumulating in heaps or clumps of various size and shape. The individual blood-corpuscles frequently appear swollen and cloudy, and their contours less distinct than natural. Small corpuscles, one-third of the normal size, are often met with, some of which exhibit a more intense color than natural, whilst others are completely pale. In the interspaces of the clumps of red corpuscles, great numbers of white corpuscles may be seen, often coalescing to form groups of from 3 to 8. In typhus he counted from 60 to 80, and more, in one field of vision; in cholera from 110 to 130. Careful enumeration of the relative numbers of white and red corpuscles four days after death in the above cases showed that there was 1 white to 60 red corpuscles in the case of typhus, and 1 white to 23 colored in the case of cholera. In a very anæmic woman, suffering from suppuration in the knee-joint, the proportion of the white rose to 1 to 13 red. The white corpuscles in these cases presented unusually active and extensive amœboid movements. The nuclei of the colorless corpuscles took a part in the amœboid movements, and could be seen altering their position and form in the interior of the white corpuscles. The thorn-apple or horse-chestnut-like form of the red corpuscles he did not find to be unusually frequent. He found, however, large quantities of granular or detritus-like material in the blood of febrile, but not much in the blood of cachectic and anæmic patients. From his enumerations he feels satisfied that in febrile diseases, and in Bright's disease, the conversion or development of white corpuscles into red is either materially retarded, or is entirely arrested.—*London Lancet*, Jan., 1875.

PHOSPHIDE OF ZINC.—At a recent meeting of the Obstetrical Society (*Lancet*, Feb. 27) Mr. J. Ashburton Thompson communicated a short paper exemplifying the advantage of employing phosphide of zinc in the treatment of chlorosis and anæmia. It succeeded in relieving the symptoms where iron had failed, and that rapidly. Phosphorus was of great value in the treatment of patients recovering from uterine hæmorrhage, and in all cases of anæmia, and seemed to exercise a specific influence upon the neuralgia so often met with in these cases, encouraging the general nutrition of the body. Free phosphorus is not the treacherous poison it has hitherto been considered to be. It is a fatal and potent poison, it is true, but its therapeutic effects may be obtained with precision and perfect safety. If proper formula be employed, no apprehension of unexpected or uncontrollable poisonous effects of a therapeutic dose need hinder its general employment.

Dr. Routh said it might be known to some members of the Society that he had made several inquiries on the employment of phosphorus. In some cases it produced marvels, especially in cases like those detailed by Mr. Thompson, due to deficiency of phosphorus in the system. But it occasionally acted injuriously, by producing headache and giddiness; and in a few cases (of idiosyncrasy perhaps) it acted as a deadly poison, even in the first dose, sometimes immediately producing vomiting and syncope. The safest preparation was the phosphide of zinc; but it was a medicine always to be closely watched when given.

Dr. Tilt inquired what preparation of phosphorus Mr. Thompson recommended.

Mr. Thompson, in reply, said Dr. Routh had revived the old objection, not because it was a poison, but because we cannot calculate the results. These arose from decomposed phosphorus.

Dr. Playfair inquired if the zinc phosphide was not insoluble in pill.

Mr. Thompson replied that some acid tonic given simultaneously would serve to assist in the decomposition. —*Phil. Med. Times.*

SALICYLIC ACID.—The *Boston Medical and Surgical Journal*, March 11, contains an abstract of a letter received by Prof. Horsford from Kolbe, of Leipsic, in which the latter gives various details of interest relative to the use of this product. In the lying-in hospital of Leipsic it seems to have superseded carbolic acid as a disinfectant for the hands, vaginal douching, application to ulcera puerperalia, etc. It is used in solution in water of one part in three hundred to one part in nine hundred, or as a powder mixed with starch in proportion of one part in five. Kolbe recommends the trial of this remedy internally in the various zymotic diseases, in pyæmia, bites of dogs, etc. In order to prove its innocuousness, he administered to himself and a number of students various doses of the acid, up to twenty-three grains a day. The following is recommended by Wunderlich as a convenient formula for its administration:

℞	Acidi salicylici,	gr. xvi;
	Ol. amygdalæ dulc,	f ʒ vi;
	Acaciæ gummi,	ʒ iii;
	Syrupi amygdalæ,	f ʒ viiss;
	Aq. aurantii flor., ad	f ℥ iv.—M.

Salicylic acid is very little, if at all, absorbed through the skin.— *Phil. Med. Times.*

IODINE AS A SUBSTITUTE FOR IODIDE OF POTASSIUM.—It is found that in some cases, where iodide of potassium is not tolerated by the stomach, iodine itself can be administered in the form of pills.

The iodine is first suspended in albumen, and afterward the combination is dried and powdered. In this way one-quarter grain of iodine can be made into a pill, and two and a half grains can be given daily, without disordering the stomach. This method was obtained from Paris, and proves of much service in relieving tertiary symptoms.—*N. Y. Medical Journal.*

ON THE EFFECT OF VENOUS OBSTRUCTION OF THE SKIN.—Dr. Auspitz has lately brought before the k. k. Gesellschaft der Aerzte in Vienna, some observations upon the effect of venous obstruction of the skin, which are reported in the *Allgemeine Wiener Medizin. Zeitung* for November 3.

1. A ligature was tied round the arm of a healthy person, as if for bleeding. First the superficial veins swelled, then a livid color spread over the arm, beginning on the flexor surface (where the skin is thinnest), and at last affecting all but the volar eminence (the thickest part of the skin). At the same time the temperature sank. The next effect was œdema of the skin, followed in from five to ten minutes by the appearance of numerous patches of red or brownish-red color, accompanied by minute spots, which were either bright scarlet or purple in tint. On removing the bandage, the cyanosis first disappeared, then the œdématous swelling; next the red patches gave place to a diffused blush of the whole arm; and lastly this disappeared, leaving only the minute red spots, which remained for several hours or even days. Comparing these appearances with those observed by Cohnheim in a rabbit's ear as the result of mechanical venous obstruction, there can be no doubt that the minute red spots above mentioned are extravasations, either of red blood-corpuscles or of their hæmoglobin in solution.

2. Similar experiments were next tried by Dr. Auspitz on the arms of persons suffering from measles, variola, and other cutaneous diseases.

a. In the cases of measles, it was observed that the large red patches above mentioned coincided with those of the eruption; and that beside the minute ecchymoses, larger spots of cutaneous hæmorrhage sometimes appeared.

b. The effect in cases of urticaria was less marked. The wheals were more prominent, and minute ecchymoses were not more frequent (perhaps less so) than in the normal skin.

c. In ordinary small-pox, there was intense congestion of the bases of the pustules, and the points of cutaneous hæmorrhage were both more numerous and larger than in a normal arm; but there was never any extravasation of blood in the pustules themselves.

d. In cases of hæmorrhagic variola, the whole of the arm below the ligature became rapidly covered by a dark-blue lividity, which concealed all minor shades of color. This was the case even when there were few or no pustules; and when these were present, they were not themselves the seat of extravasation of blood.

e. The ligature applied to the arm of patients suffering from scarlatina, had little or no effect beyond that observed in the experiments above noted on the normal skin.

f. In "scorbutic affections—*erythema nodosum*, *morbus maculosus Werlhoffii*, *purpura rheumatica*, and scurvy proper"—Dr. Auspitz was surprised to find the effect of the ligature trifling. There was no scarlet injection of the skin and no ecchymosis. This seems to confirm the old belief (recently called in question by Cohnheim) that purpura depends on a change in the blood itself, and not on any difference of pressure in the circulation, or on anatomical lesions of the blood-vessels.— *London Medical Record*, December 30, 1874.

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